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THE DIEPPE RAID CASE

A COLLECTIVE AUDITORY HALLUCINATION

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ON the 4th August, 1951, two English ladies, Mrs Dorothy Norton (pseudonym) and her sister-in-law Miss Agnes Norton (pseudonym), while staying for a holiday at Puys (Puits) near Dieppe (France), had a remarkable experience in the nature of a collective auditory hallucination. In order to safeguard those concerned against unwanted publicity, the real names of the percipients and the exact address at which they were staying are not given in this report. For the sake of brevity the two percipients are referred to below as D. and A. respectively. Their ages at the time were thirty-two and thirty-three.

D., accompanied by her two young children and their nurse, arrived at Puys on the 26th July, 1951, A. having arrived on the preceding day, for a visit lasting till August 5th following. Puys was chosen for the holiday by D. and her husband during a three-day visit to Dieppe at Easter 1951. Neither percipient had any particular interest in what happened at Dieppe during the Second World War. Both had read in the press about the Dieppe Raid of the 19th August, 1942, at the time of its occurrence, but had not looked up the history of it in connexion with the visit. They have been friends since childhood, but had not previously been on a holiday together.

The percipients' Statements recording what they heard are reproduced below. They are based on notes taken during the experience, which, it will be seen, lasted nearly three hours, from about 4 a.m. till about 7 a.m. The Statements were prepared from the notes partly on the same day (4th August) and partly on the following day, before the party left for England. At D.'s suggestion the notes were written independently. The Statements were posted to the Society by D. with a covering letter dated 9th

August, written after her return. This letter merely described the circumstances in which the experiences took place, and inquired whether the Society had had any other reports of this kind. The writer of this report has seen the notes from which the Statements were prepared, and has checked the Statements against them.

At Puys D. and A. shared a bedroom on the second floor of a three-storey house facing towards the sea, which was about a quarter of a mile away, down a steep path. The house had, so they were informed, been used as quarters for German troops during the war. The nurse and two children, who heard nothing unusual, were in another bedroom on the same floor, two doors away. The times mentioned in the Statements were taken by D. and A. from their wrist watches. Both percipients agree that A.'s watch was keeping better time than D.'s, which was losing slightly. A. was in the Women's Royal Naval Service during the war, and became accustomed to the accurate recording of time. A.'s time is therefore to be preferred, where there is any discrepancy. Both watches were set to 'Single Summer Time' (one hour ahead of G.M.T.), which was also in use on the 19th August, 1942, for service purposes in the Allied Forces and for civil purposes in France. The weather was fine throughout the experience.

1. Copy of statement by Mrs Dorothy Norton

Saturday, August 4th, 1951

At 4.20 a.m. A. got up, and went out of the room. I said 'Would you like to put the light on?', but she didn't. She came back in a few minutes. She said, 'Do you hear that noise?' I had in fact been listening to it for about 20 minutes. I woke up before it started. It started suddenly and sounded like a storm getting up at sea. A. said she had also been listening to it for about 20 minutes. We lay in the dark for a little listening to the sound. It sounded like a roar that ebbed and flowed, and we could distinctly hear the sounds of cries and shouts and gunfire. We put the light on and it continued. We went out on the balcony where we could look down towards the beach, though we could not actually see the sea. The noise came from that direction and became very intense, it came in rolls of sound and the separate sounds of cries, guns and divebombing were very distinct. Many times we heard the sound of a shell at the same moment. The roaring became very loud. At 4.50 it suddenly stopped. At 5.5 a.m. it started again and once more became very intense, so much so that as we stood on our balcony, we were amazed that it did not wake other people in the house. By now it was getting light, cocks were crowing and birds were singing. We heard a rifle shot on the hill above the beach.

The sounds became more distinctly that of divebombers rather than the cries and shouts we had heard earlier, although we could still hear them. The noise was very loud and came in waves as before. It stopped abruptly at 5.40.

At 5.50 it started again but was not so loud and sounded more like planes. This died away at 6 a.m. At 6.20 the sound became audible again but it was fainter than before, and I fell asleep as I was very tired.

I was woken by a similar sound on Monday, July 30th, it sounded exactly the same only fainter and not so intense. At the end I seemed to hear a lot of men singing. It ended when the cocks started crowing and I went to sleep. My sister-in-law did not waken.

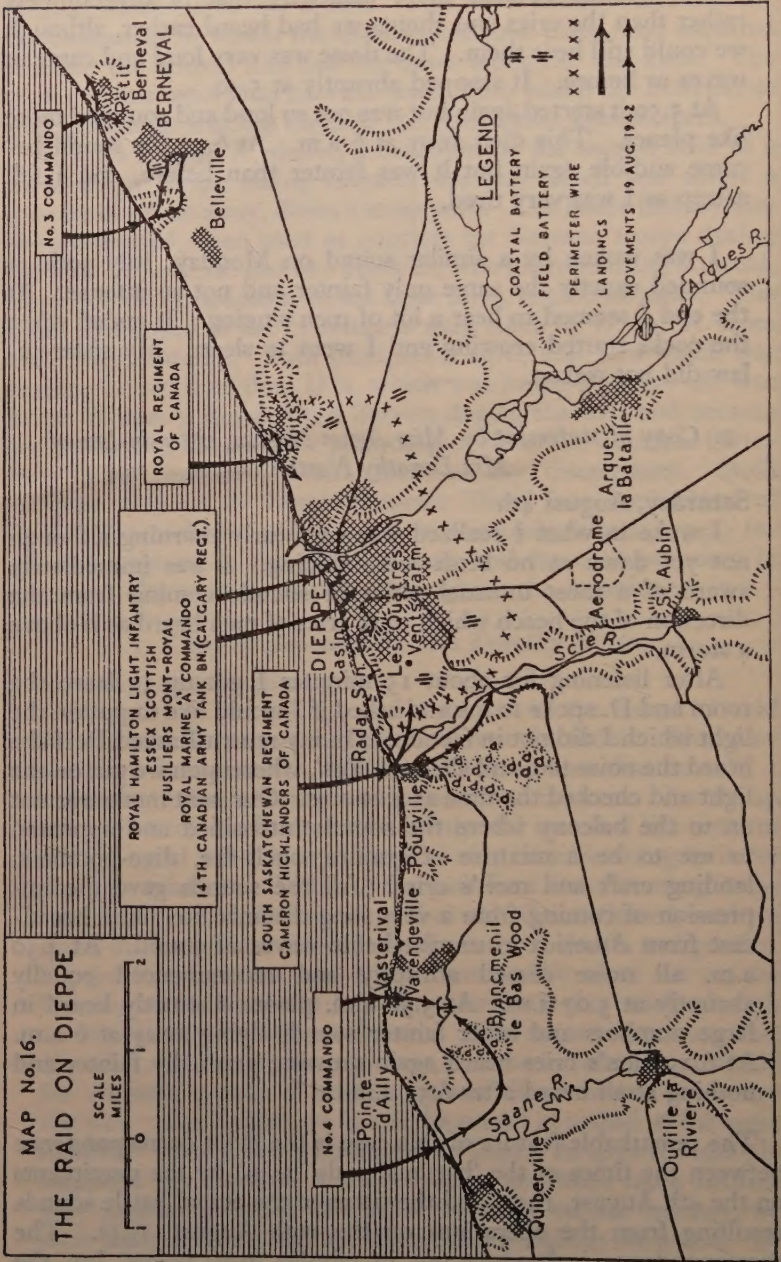
2. *Copy of statement by Miss Agnes Norton, sister-in-law of Mrs Dorothy Norton*

Saturday, August 4th

I woke in what I realized was very early morning although not yet dawn as no birds were singing. I was immediately aware of a most unusual series of sounds coming from the direction of the beach which were cries of men heard as if above a storm.

After listening for about 15 minutes I got up to leave the room and D. spoke to me and asked if I would like to put on the light which I did not in fact do. On my return I asked D. if she heard the noise too, and she said 'Yes', whereupon we put on the light and checked the time as 4.20 a.m. Our next move was out on to the balcony where the sounds intensified and appeared to me to be a mixture of gunfire, shell-fire, dive-bombers, landing craft and men's cries. All the sounds gave the impression of coming from a very long distance, i.e. like a broadcast from America in unmistakable waves of sound. At 4.50 a.m. all noise ceased abruptly and recommenced equally abruptly at 5.07 a.m. At 5.50 a.m. planes distinctly heard in large numbers and other fainter sounds dying away at 6 a.m. At 6.25 men's cries heard again growing gradually fainter and nothing at all heard after 6.55 a.m.

The remarkable feature of this case is the close correspondence between the times of the 'battle sounds' heard by the percipients on the 4th August, 1951, and the times of the actual battle sounds resulting from the operations on the 19th August, 1942. The correspondence is brought out in tabular form below, but the reader will be in a better position to appreciate the comparison if



he is first given a brief outline of the plan and course of the Raid. For fuller accounts, see the section on Dieppe in the volume *Norway—The Commandos—Dieppe*¹ by Christopher Buckley, which was published in January 1952, some five months after the experience of the two percipients, and *The Canadian Army 1939-45*² by Colonel C. P. Stacey.

At Dieppe, as will be seen from the accompanying map, the French coast runs, roughly speaking, east and west. The plan was first to land small forces by surprise at places on either side of Dieppe to destroy coastal batteries. These flank landings, as they were called, were directed to Puys, about $1\frac{1}{2}$ miles east of Dieppe, and to Berneval, about 5 miles further east; also to Pourville, about $2\frac{1}{2}$ miles west of Dieppe, and to Varengeville about 3 miles further west. All these flank landings were due to take place at 4.50 a.m., to be followed half an hour later, at 5.20, by the main landings at Dieppe itself. The main landings were to be preceded and covered by a bombardment of Dieppe by the destroyers accompanying the force, and by air attack.

In the event the flank landings west of Dieppe took place punctually. They met with so little opposition on the beaches that they were doubtless hardly heard at all at Puys, on the other side of Dieppe. On the east, the allied vessels closing on Berneval ran accidentally across a small protected German convoy at 3.47 a.m., when about seven miles from the French coast. Firing started immediately after between the opposing vessels, and the noise gave about an hour's warning to the Germans, anyhow on the east side of Dieppe. The accounts of the engagement do not show exactly when this firing stopped, but there is evidence that it lasted till after 4 a.m.

Meanwhile the vessels closing on Puys moved on, arriving 17 minutes later, i.e. at 5.07 instead of at 4.50. (This time, 5.07, has been established as more correct than 5.05, the time indicated for this landing in a report by the Naval Commander, Captain J. Hughes-Hallett, R.N., dated 30th August, 1942, published on 14th August, 1947, as a Supplement to the *London Gazette* of the 12th of that month. That report makes the time 15 minutes late on 4.50, i.e. 5.05.) The Germans, who had had time to man their beach defences, waited until the landing craft had almost touched down and then opened heavy fire on the disembarking Canadian troops. A second 'wave' went ashore a few minutes later, followed by a further landing at about 5.45. Those troops who succeeded in getting ashore were confronted by a high sea-wall which gave little shelter against a murderous enfilading fire from enemy pill-

¹ London, H.M. Stationery Office, 1952.

² Ottawa, The King's Printer, 1948.

boxes. Very few penetrated inland, and at 8.30 a.m. the survivors of those pinned to the beach surrendered. Buckley (p. 249) sums up the situation in these words :

In the space of two or three hours the Royal Regiment of Canada had been practically exterminated. Of 26 officers and 528 other ranks who had embarked for the operation on the previous night, all the officers and 496 other ranks became casualties. Two officers and 65 others, half of whom were wounded, were brought away ; no less than eight officers and 199 others were killed ; 16 officers and 264 others, wounded and unwounded, fell into German hands. . .

To the noise of machine-gun and rifle fire from positions commanding the beach there was added the sound of firing from a German howitzer battery, situated a few hundred yards south of Puys, which was directed at the allied shipping lying off the shore. The noise of the shells passing overhead must have been audible to persons in the house in which the percipients found themselves nine years later. German records show that the number of shells fired during the course of that morning by the battery in question was 550.

At 5.12 the destroyers accompanying the main force started to bombard Dieppe, preparatory to the landing there, and, just before that landing started, beginning at about 5.15, Hurricanes made a cannon-firing attack on the sea-front buildings. The landing itself was met by a heavy outburst of firing from the shore. At the same time (5.20) air cover was increased to its maximum, but the increasing noise of aircraft was probably masked for the time being by the naval gunfire.

At 5.40 the naval bombardment stopped, and the sudden drop in the amount of noise audible at Puys must have been very noticeable. On the other hand the sound of aircraft must have become much more pronounced, rising to a maximum at 5.50 a.m., when there was a change-over of the air cover, forty-eight fighters arriving from England to relieve an equal number due to return there. By that time, too, there was a considerable increase in the number of German aircraft overhead.

From 5.50 to 6.55 there was so much activity at several points of attack that one cannot estimate the result in the way of noise, as heard at Puys. It is only necessary to add that there was a considerable amount of noise after 6.55 (when the experience ended), including a good deal of gunfire from naval vessels covering the re-embarkation of allied troops.

In the following table the left-hand column divides the experience into phases (Roman numerals), and under each phase brings together corresponding extracts from the Statements of the two

percipients. The right-hand column shows the officially recorded times of relevant events at the same hour of the day on the 19th August, 1942, and cites sources published before the 4th August, 1951. After 6 a.m., as stated above, the increasingly complex pattern of events makes it hardly possible to continue the comparison. If the reader should feel that, even so, the amount of correspondence between the columns may have been exaggerated by 'unfair' selection of events for the right-hand column, his best recourse would be to read the recently published account of the Raid in *Norway—The Commandos—Dieppe*, bearing in mind that it was not accessible to the percipients, and to study the course of events against the wider background there presented.

COMPARATIVE TABLE

*Percipients' Statements**Events on 19th August, 1942*

I. About 4 a.m. (i.e. about 20 minutes before 4.20).

D. 'It [the noise] started suddenly and sounded like a storm getting up at sea. . . . It sounded like a roar that ebbed and flowed . . . sounds of cries and shouts and gunfire.'

A. '... unusual series of sounds coming from the direction of the beach which were cries of men heard as if above a storm.'

II. 4.50 a.m.

A. '... all noise ceased abruptly.'

D. '... suddenly stopped.'

III. 5.07 a.m.

A. '... recommenced abruptly at 5.07 a.m.'

3.47 a.m. Assault vessels closing on Berneval ran across German convoy. Firing began immediately after, and went on until after 4 a.m.

Note. This time (3.47 a.m.) was published in several accounts, e.g. *Combined Operations*¹ and *The Green Beret*² by Hilary St G. Saunders (p. 104).

At Puits there was probably shouting by German soldiers manning the beach defences.

4.50 a.m. was zero hour for the flank landings, which, at Berneval and Puits, were delayed. There may have been silence at Puits at this stage.

Several published reports mention 4.50 as zero hour but there is nothing to suggest that silence fell just then.

5.07 a.m. The first wave of landing craft touched down at Puits in the face of heavy fire.

¹ London, H.M.S.O., 1943.

² London, Joseph, 1949.

Percipients' Statements

D. 'At 5.05 a.m. it started again and once more became very intense. . .'

Note. This discrepancy of two minutes is discussed below.

D. 'The sounds became more distinctly that of dive-bombers rather than the cries and shouts we had heard earlier. . .'

IV. 5.40 a.m.

D. 'It [the noise] stopped abruptly at 5.40.'

(*Note.* A. does not mention this.)

V. 5.50 a.m.

D. 'At 5.50 it started again . . . and sounded more like planes.'

Events on 19th August, 1942

Note. This time was published by inference in *The Canadian Army* 1939-45 cited above. The landing of the first wave is recorded as having been 17 minutes late—i.e. 4.50 plus 17 minutes (p. 71). Buckley (p. 247) also puts the delay at 17 minutes.
5.12 a.m. Destroyers started to bombard Dieppe.

5.15 a.m. Low-flying Hurricanes attacked the sea front buildings.

5.20 a.m. The landing of the main force at Dieppe began, in the face of heavy fire.

5.40 a.m. The naval bombardment of Dieppe stopped.

Note. A press correspondent in *The Times* of 21st August, 1942, stated that the bombardment continued for 20 minutes after the landing had begun (i.e. till 5.20 plus 20 minutes).

5.50 a.m. Forty-eight R.A.F. aircraft arrived from England (see p. 612). 'By now, shortly before 6 a.m. . . the noise of aeroplanes had risen to a constant drone, like a net of harsh sound under the sky.' (Same correspondent as under IV above.)

The only account of the Raid which was in the hands of the percipients at the time was one contained in a French guide book entitled *Dieppe*, a copy of which was handed to K. G. by D. They say that they knew of the existence of this account, but had not read it before the experience started. After the noise had begun, they read it on the balcony at about the middle of Phase III. It is therefore necessary to examine carefully passages in the account from which clock-times up to about 7 a.m. might have been obtained. They are as follows :

(1) Le 19 août 1942, la population côtière fut réveillée par une canonnade en mer, au large de Berneval ; à 5h.50, une multitude

d'avions se précipitaient sur Dieppe et ses environs, pendant que des flotilles sous la protection de destroyers s'approchaient des côtes et débarquaient en huit points différents de Sainte-Marguerite à Berneval, des contingents alliés comprenant près de 7000 hommes. (p. 30).

No reader not already acquainted with the facts would realize that the firing off Berneval referred to at the beginning of the above passage took place two hours before the time 5.50 mentioned immediately after the semi-colon. Nor, from the use of the expression 'pendant que' would he be able to infer that the flank landings had already taken place, and that the main landings were well advanced by 5.50.

(2) A Puy, par suite de la fausse manoeuvre d'une flotille, le débarquement fut retardé et fut effectué comme le jour se levait. (p. 41).

This statement is correct, but the reference to the time of the landing is too vague to account for the precise time 5.07 coming out in A.'s statement.

(3) L'attaque principale se produisit à Dieppe. A 5h.20, les destroyers alliés ouvraient le feu. (p. 41).

According to a naval report, it was at 5.12 that the destroyers opened fire. 5.20 was the zero hour for the landings at Dieppe, and they started within two or three minutes of that time.

(4) Vers 7 heures, une vingtaine de tanks du 'Calgary Regiment' ... débarquèrent, sauf deux. . . qui furent coulés. (p. 43).

These tanks arrived in vessels which touched down between 5.35 and 6.05 and, apart from two which were sunk, were all on shore soon after 6 a.m.

It will be seen that the percipients' time-table could not have been got from the French account, except perhaps the time 5.50 for the sound of aircraft at Phase V. Both percipients are sure that they had never heard of the existence of any of the reports and accounts mentioned above, apart from the French guide book and anything they may have read in the press at the time of the Raid; and certainly had never read the Hughes-Hallett report or the account in the Canadian volume. A. has also assured us that during her service in the Women's Royal Naval Service she was not in a position to see unpublished naval reports of operations. Between the 4th and 9th August, when D. wrote to the Society, neither percipient appears to have attempted to correlate the times given in her notes with actual battle times.

Even if both percipients had noticed before 5.50 a.m. the statement in the French guide book that a large number of aircraft flung themselves on Dieppe at that hour on the 19th August, 1942,

that knowledge would not ordinarily have caused hallucinatory hearing of aircraft noise by two persons at that particular time. It seems reasonable to attribute the 'appropriate sounds' heard at 5.50 to the same cause as those heard earlier at about 4 a.m. and at 5.07 a.m.

There are some details in the Statements which do not link up with any ascertainable facts about the Raid, and for that reason cannot be attributed to or checked by reference to accounts of the operation. For instance, there is nothing in the accounts to suggest that noise ceased suddenly at Phase II; nor that noises died away at 6 a.m.; nor, as A. says, that men's cries were particularly audible at 6.25. There is nothing inherently improbable in any of the statements, if they are read in relation to happenings audible at Puy at corresponding times during the Raid.

Phase III, which furnishes the most remarkable coincidence in the whole series, provides also a very curious puzzle. Both percipients are sure that they simultaneously heard the noise start again, but at this point there is a discrepancy of two minutes in their timing. They attribute this to D.'s watch having been two minutes slow on A.'s watch, which, as stated above, is more likely to have been right than D.'s. All other times noted by both percipients end in 5 or 0, and it may be that A. was reading times to the nearest minute, whereas D. was reading them to the nearest 5 minutes; also, that 5.07 was the only time 'clocked' which did not, to the nearest minute, end in a 5 or 0. Not to labour the point unduly, the argument favours the conclusion that for the left-hand column of the Comparative Table 5.07 is more accurate than 5.05 for the beginning of Phase III.

Oddly enough, there is a discrepancy of the same two minutes in the reports of the time at which the landing began at Puy on 19th August, 1942. Captain Hughes-Hallett, in paragraph 12 of his report, makes the time 15 minutes late on 4.50 (i.e. 5.05) but admits in effect that his information about that landing was subject to correction in the light of a separate report from another officer who was in a better position to know what happened. The time accepted as correct by the historians of the Raid is 17 minutes late, i.e. 5.07, and the discrepancy is clearly due to the fact that two different observers, one in a better position than the other, judged differently the time at which the landing began. There accordingly appears to be no connexion at all between the discrepancies as such, but as the clearing away of the discrepancies leaves us with a 'corrected' time 5.07, on each side of the Table, we are faced with a coincidence which seems far outside the bounds of chance. No other landing than that at Puy is recorded as having started at 5.07, and that minute stands out with grim

significance in the history of what was known as the 'Blue Beach' landing.

CONCLUSION

As many supposed auditory hallucinations turn out on investigation to be cases in which some ordinary noise has been misinterpreted, it is perhaps necessary to point out that that explanation will not serve here. Any theory that the noises heard were due to water in pipes, or to artillery practice a long way off, the sound of which had been carried to Puys by some freak effect, would fail to explain why no one else heard the noises. The percipients say that they inquired during the day (4th August) of several persons whether they had been disturbed during the night by any unusual noise, and received negative answers. In particular, they asked a fellow visitor who had repeatedly complained of being disturbed at night by casual noises, as they had seen her bedroom light on when they were standing on the balcony listening to noises of 'amazing' loudness. She said she had not heard anything unusual. Nor, it may be added, could the noises have come from a cinema running through a film at an unusual hour, for there is no cinema in Puys.

On the other hand, it would, in our opinion, be rash to assume that the sounds heard were a sort of 'sound track' repetition of the sounds of the Raid. The various kinds of sounds heard, gunfire, dive-bombing, planes, a rifle shot, shouts and cries, are all appropriate, but there is not enough detailed information available as to when the several kinds of sound first occurred to enable one to judge whether they are 'phased in' correctly.

It will be seen from the last paragraph of D.'s Statement that she had a slighter and much shorter experience of the same kind on the morning of 30th July, which was not shared by A. who was asleep. D. says she did not mention this to A. till about two days later, as she (D.) was not in the mood to have her holiday interfered with by 'uncanny' happenings, and when she did mention it to A. neither of them thought it sufficiently mysterious to be worth pursuing. This, followed by the much more 'successful' performance on 4th August, suggests that some resistance had to be overcome to 'put across' the sound effects, and they may have been representative, rather than exact repetitions, arranged in a manner that stressed certain clock-times by preceding intervals of silence.

D. had had three previous experiences of a 'psychic' nature, but none of them was a purely auditory hallucination, so she had no reason to expect an experience of that particular kind at Puys. Two of these earlier experiences were visual hallucinations which,

though involving an apparition of a person well known to her, would not necessarily be considered evidential. The third took place one night during her visit to Dieppe at Easter. It, too, was not evidential. Consisting of a peculiarly powerful impression—possibly a dream—of someone being chased through her bedroom and towards the window, it is mainly of interest here because one might possibly associate it with events which took place in Dieppe during the Raid of August 1942, though this does not seem to have occurred to the percipient. A. has never had any other psychic experience, and it seems likely that the presence of D. had something to do with her hearing hallucinatory sounds at all on 4th August.

We have been impressed by the commendable pains taken by the percipients to record the evidence at once, on a day when they must have been busy preparing to return to England, and by the candour with which they have answered questions we have put to them. They both seemed to the investigators to be well-balanced individuals, with no tendency to add colour to their accounts. Neither of them has shown any concern whatever to 'prove' by the experience any preconceived theory of its cause, which would have been likely to determine the form it took. Both as regards form and content we think the experience must be rated a genuine psi phenomenon, of which little or nothing was derived from previous normally acquired knowledge.

The accounts of the Raid in this report are based mainly on information contained in the published sources to which references are given, notably *Norway—The Commandos—Dieppe* by Christopher Buckley and *The Canadian Army 1939–45* by Lt. Col. C. P. Stacey. For advice as to sources the writer of this report is indebted to the Librarian of the War Office, and for certain details about the movements of aircraft to the Librarian and Archivist of the Air Ministry. The Controller of Her Majesty's Stationery Office has kindly given permission for the reproduction of the map and for the quotation on p. 612 from *Norway—The Commandos—Dieppe*.

G. W. L.

ESP TESTS WITH PSYCHOTICS

BY D. J. WEST

Summary

THREE series of tests¹ carried out over the last year are here recorded. The first was exploratory, the second and third were designed to test whether extremely hostile and suspicious attitudes, commonly found in certain types of mental patient, would be conducive to negative scoring. In none of the three series was there found any clear evidence of an ESP effect. Some of the practical difficulties encountered in administering ESP tests to psychotic patients are described.

Introduction

It is often assumed that insane or peculiar people are likely to make good psychic subjects. Certainly psychotic patients often believe themselves to be possessed of special psychic gifts. Ehrenwald (1) has gone so far as to base a theory of schizophrenia on the supposition that these sufferers are in fact being overwhelmed by an uprush into their consciousness of a chaotic mass of extrasensory impressions.

Urban (2) has carried out many card tests with psychotic patients and has reported strikingly positive scores as well as significant differences in scoring level between several diagnostic groups. This work awaits confirmation. In my view his results are unreliable on account of inadequate experimental precautions. In 1937 Shulman (3), in some tests on psychotics in the Hudson River State Hospital, obtained an overall score close to chance expectation. The results from one of his fourteen diagnostic groups—the manic-depressive depressed—give a significant positive deviation ($CR=3.39$). This is suggestive of an ESP effect, in spite of the obvious selection factor, but there were not enough patients in his series to demonstrate that the results of the manic-depressive depressed were significantly greater than what might have come about by variation in the patients' scoring levels independently of diagnosis.

In 1938 Margaret Price (4), of the Duke University Parapsychology Laboratory, carried out a large series of ESP card tests with 50 mental patients in the Ohio State Hospital. A report was not published until thirteen years afterwards, since 'the work did not meet the more stringent experimental require-

¹ My thanks are due to the authorities of the Bethlem Royal and Maudsley Hospital, Horton Mental Hospital, Surrey, and Northumberland House, Finsbury Park, where the tests were carried out.

ments of that time, and it had little chance for immediate publication'. In 1700 runs of high aim trials the patients achieved an average score of 5.54 ($CR=11.05$, $P=10^{-28}$) and in 495 runs of low aim the average score was 4.65 ($CR=3.89$, $P=10^{-4}$). The effect was a fairly generalised one, found in patients of all diagnostic types. A breakdown into co-operative, apathetic, and irritable patients showed that both the first two categories scored significantly better than the last one. Thus the attitude of the patients tested had considerable influence upon the results obtained, whereas the nature of their mental derangement did not seem to make much difference.

Dr Betty Humphrey and other American experimenters have recently carried out further ESP tests with mental patients, but their findings have not yet been published.

Series I

The first series of card tests was entirely exploratory, intended to discover any special peculiarities in the results that might be obtained from a group of unselected psychotics. Twenty-five patients were tested, comprising eighteen schizophrenics, one schizo-affective disorder, five psychotic depressions, and one case in which the diagnosis proved doubtful. The patients were selected solely on the basis of their availability and their being neither too disturbed nor too uncooperative to be testable.

Each subject was given four runs of DT trials with the standard ESP cards. The cards were randomised, arranged in packs of twenty-five, and enclosed in numbered envelopes by an assistant. The experimenter presented the envelopes to the subject one at a time, asking him to call twenty-five guesses at the order of the pack from top to bottom inside the envelope. After every twenty-five calls, the target envelope was opened, and the guesses checked in front of the subject. Since there was often no observer present, it was an important condition that the experimenter did not know the order of the target cards until after he had written down the subject's calls, and that the experimenter's record of the target order was later checked against the record that had previously been made by the assistant who randomised the cards.

The results obtained showed a close agreement of total score (both in direct hits and displacement hits) with chance expectation :

	-1	0	+1
Observed hits	477	512	451
Expected hits	480	500	480

There was no significant scatter of subjects' scores. The largest deviation obtained was by Mr T., who scored negatively. The

next largest negative deviation was produced by Mrs C. These two subjects were exceptional in that both were highly paranoid and both fitted the experimenter into their delusory systems in a persecutory role. Mr T., who was one of the experimenter's own patients, believed that the experimenter was responsible for the 'voices' that plagued him all day long. Mrs C., although she was unknown to the experimenter before the test, 'recognized' him at first sight as someone from South Africa who was concerned in a plot against her. Their scores were as follows :

TABLE OF SCORES

	1st run	2nd run	3rd run	4th run	Deviation	C.R.
Mr T	2	3	1	4	- 10	- 2.50
Mrs C	3	1	5	5	- 6	- 1.50
Total					- 16	- 2.8

This result suggested that a highly hostile and suspicious attitude might be connected with negative scoring. It was with this suggestion in mind that a second series of tests was carried out.

Series II

The purpose of this series was to test the hypothesis that hostile, paranoid patients would tend to produce negative scores. Twenty-two subjects were tested. (It was originally planned to try fifty subjects but, owing to the experimenter leaving the hospital, and the experiment proving time-consuming and apparently unproductive, the series was interrupted.) All the subjects were psychotics with paranoid delusions ; most of them were suffering from chronic paranoid schizophrenia. A difficulty was encountered in that the most hostile and most paranoid patients refused to have anything to do with the test, and some of those who were included in the series did not exhibit overt suspicion or hostility in the test situation. The method of testing used was DT clairvoyance, with four runs per subject, exactly as in Series I.

The results were completely null, there being no evidence of any negative trend and no significant displacement effects :

	- 1	0	+ 1
Observed hits (23 subjects)	442	459	433
Expected hits	422.4	440	422.4

Five of the subjects who were noted to be especially hostile did not produce outstanding scores. Series II included a retest of Mr T., who had produced the largest negative deviation in Series I,

but this time his score was close to chance. It must be admitted, however, that in no case in Series II, not even in the retest of Mr T., did the subject express the delusion that the experimenter was a persecutor.

Series III

This series consisted of a group test which, like Series II, was designed to investigate the effect on scoring of marked hostility and suspicion in the subjects. Dr F. K. Taylor of the Maudsley Hospital supplied the subjects. They consisted of six patients who met regularly once a week with a doctor in order to receive psychotherapy as a group. All of them were highly paranoid, several were definitely deluded and psychotic, and the rest were borderline cases. All of them felt persecuted, and tended to be aggressive and suspicious in their dealings with other people. D. J. West sat in on one of their weekly sessions, listening in silence and taking notes while they discussed their intimate affairs with their doctor. At the end of the session, when it may be safely assumed they were feeling peculiarly resentful and suspicious, they were required to take part in an ESP test.

The test consisted of eight runs, DT clairvoyance, under the same general conditions as in Series I and II, except that the subjects wrote down their guesses, all at the same time, and all aiming at the same target. No significant trend was found in the results :

<i>Subject</i>	<i>No. of Runs</i>	<i>Deviation</i>
A	8	+3
B	8	-6
C	8	+1
D	4	+2
E	8	+3
F	8	+3
Total	44	+6

A Note on the Practical Difficulties of Administering ESP Tests to Psychotic Subjects

In the investigation of possible correlations between the mood of the subject and ESP scoring, psychotic patients provide useful material, since in them moods such as aggressiveness, suspicion, elation, or apathy, occur spontaneously in forms more extreme than are ever likely to be encountered in normal people. Unfortunately, largely because of these violent moods, psychotics are often uncooperative or inaccessible to the experimenter. Even when they appear superficially to agree to co-operate in the ESP test, they often succeed in resisting by indirect means. They

sometimes keep up endless arguments about whether it is right for them to take part in the test, or try to put off the test to another day. Others take so long over their calls, trailing off into such protracted discussion of the pros and cons of each choice, that the experimenter's patience becomes exhausted. Another, and even commoner habit, is to employ systematised guessing. In an extreme case they may say 'Let's have them all crosses' or 'five circles, then five waves, then five stars, then five squares and then five crosses'. More commonly they call the cards in sets of five, e.g. 'cross, square, star, wave, circle', repeating the sequence over and over again either in the same or reversed order. The persistent use of one or other rigid system of calling, after the experimenter's repeated requests to guess the cards at random, is such a common feature of psychotic behaviour in the ESP test situation that it could almost be used as a diagnostic aid.

Systematic card-calling presumably tends to prevent any genuine ESP response, and should therefore be discouraged. It will be remembered that when Dr Soal's subject, Basil Shackleton, adopted fixed guessing habits, he did not score above chance. But no matter how far from random the subject's calls may be, spurious extra-chance scores cannot be produced provided the target cards are strictly randomised.

The chief disadvantage in the use of psychotic patients as subjects is the great amount of time taken up in making contact with them and in conducting the tests. It was not uncommon to have to spend a whole afternoon in obtaining a subject willing to co-operate and in persuading him to complete four runs, which, with a normal subject, could easily be completed in a quarter of an hour.

While the subjects' guesses have throughout this report been referred to as 'calls', in practice it was found necessary to arrange samples of the target cards in front of them and ask them to indicate their choice with a pointing finger. Many psychotics were prepared to co-operate to this extent who would not have been prepared to call out audibly the names of the cards.

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THE SOCIETY'S NEW PRESIDENT

FOR the year which marks the Society's seventieth anniversary, the Council invited its most distinguished member, Dr Gilbert Murray, O.M., to accept the Presidency. He was formally elected on 23 April.

Dr Murray joined the Society in 1894, twelve years after its foundation, and was President for the year 1915-16. He was a member of the Council for many years, and has been a Vice-President since 1920. His Presidential Address will be delivered at 8 p.m. on Wednesday, 21 May, in the Great Hall of the Caxton Hall, Westminster, S.W.1.

REVIEWS

THE PSYCHOLOGY OF THE OCCULT. By D. H. Rawcliffe. London, Ridgway, 1952. 551 pp. Illus. 21s.

The main purpose of this book is to discredit psychical research (for which the author prefers to use the emotive term 'occult research') which he says is today 'assuming alarming proportions' and even 'invading the precincts of our most distinguished universities' (p. 9). It has seemed to many of us that it was better that experimental inquiry should be taking place into such matters, and that such experimental inquiry by trained researchers was the most hopeful way of sifting the true from the false. Mr Rawcliffe seems to feel no need for such sifting; it is all 'occultism' and, therefore, all false. All that needs to be done is to think of a 'rational' explanation of any apparently successful psi experiment.

There is, of course, nothing new in this position, and when Jastrow argued twenty-five years ago that the crucial experiments for extrasensory perception were not rigid enough to satisfy the experimental psychologist, he could make out a reasonable case for this opinion. The situation is different now. Experiments have been carried out under conditions which met all reasonable objections and still successes have continued. Unconscious whispering seemed at one time as if it might be a plausible explanation, and Mr Rawcliffe seems still prepared to back it strongly. But psi successes have taken place under conditions in which such an explanation is absurd, e.g. in ESP experiments over long distances, in precognition experiments, and in the Duke DT experiments in which the target cards were known to no one until the guesses were completed.

There is nothing judicial about Mr Rawcliffe's survey of the field of parapsychology. He reminds one rather of a prosecuting counsel with a weak case, concerned to create conviction at all costs. There is, for example, much abuse of the parapsychologist for 'bias and incompetence' (pp. 479 and 486). There are references to the parapsychologists as amateurs; they are contrasted with the psychologists who (according to Mr Rawcliffe) reject the findings of parapsychological experiments. The intention seems to be to suggest to the jury that Professor Gardner Murphy and Dr Schmeidler are amateur psychologists while the author is a professional. A jury sufficiently ignorant of the facts might perhaps be led to suppose that this was true.

Obviously one needs to trouble oneself less about the evidence if the jury can be convinced that all the witnesses on the other side are unreliable. Thus, on p. 323: 'Richet, Myers and Lodge all believed in the unseen spirit world and died steadfast in their belief'. This is true of Myers and Lodge, false of Richet; irrelevant in any case as a means of judging the correctness of their observations. Dalton believed in the atomic hypothesis and died steadfast in that belief, but this is not ground for rejecting his experimental work. On p. 313 we learn that evidence coming from Osty is highly suspect because psychical research was his all-absorbing interest. If all evidence as to paranormal phenomena must be suspect if it comes either from workers who believe in paranormal phenomena or who are strongly interested in it, we shall be left with little evidence. But a similar criterion would leave us with little evidence on any other branch of research.

On p. 323: 'Palladino's investigators were not really investigating her "phenomena" at all. What they were all hoping to do was to prove her "phenomena" authentic.' How does Mr Rawcliffe know this? 'The Soal experiments were first and foremost designed to establish *scientifically* the validity of a purely *metaphysical* concept' (p. 458). My own recollection of Dr Soal's attitude is that, on the contrary, what he was trying to establish was that, under properly controlled conditions, a subject in card guessing experiments could not beat the laws of chance. Being an open-minded man, he gave up that idea when the experimental evidence went against it, but it is absurd to credit him with the opposite pre-conception.

Although Mr Rawcliffe devotes a chapter to discussion of the American experiments in ESP, these are mostly devoted to discrediting the experimenters. There is no need to protect Duke parapsychological investigators against the charge of experimental incompetence. Many competent judges have examined their work critically and come to the opposite conclusion. Mr Raw-

cliffe makes no attempt at a fair appraisal of the evidence from this laboratory by considering the experiments carried out with the fullest precautions against error. There is, for example, no mention of the Pearce-Pratt series of experiments which is claimed by Rhine as the most adequate evidence for the reality of ESP. A sneer at Dr Schmeidler on p. 442, based on a criticism by Dr Soal of a minor detail in her evaluation of results, shows complete ignorance of the point of the criticism.

Dr Soal receives more adequate treatment in the next chapter. The Shackleton results, however, are explained as due to a double system of unconscious whispering by assistant-experimenter and agent. This suggestion is not altogether novel. If it is considered to be a possible explanation of the main Shackleton series of results, it is obviously not applicable to the preliminary Shackleton series in which there was precognitive guessing on a pack of cards, or to the Antwerp-London experiments with Mrs Stewart. For the Antwerp-London series, Mr Rawcliffe has to fall back on the suggestion that the conditions were such that Dr Soal had the opportunity of faking the records.

Mr Rawcliffe does not seem altogether happy about these criticisms of Soal's experiments, since he tries to strengthen them by some statistical considerations which show a curious lack of understanding. On p. 471, for example, he says that even if his other criticisms were not sound, the Shackleton series would be a weak support for Soal's conclusions since their total number is *only* 6,690 guesses which is a small number compared with the American experiments. This, of course, has nothing to do with the case; the anti-chance odds were 10^{35} to 1, which is by no means weak. He is surprised by the 200 guesses with 60 right by Mrs Stewart ten feet from the telephone (p. 472), but remarks that 200 trials is too small a number to be of any significance. This is not the case; the odds against such an excess of right answers arising by chance in 200 guesses is 2,000 to 1 which is amply significant.

If all his criticisms against the experiments fall to the ground, Mr Rawcliffe has a further line of defence in Chapter XXVIII. They could not prove telepathy because this is a metaphysical and not a scientific hypothesis. This is, of course, a mixture of sense and nonsense: the sense is less unfamiliar to parapsychologists than Mr Rawcliffe supposes. What one can do by experiment is to discover whether the knowing of something by one person can be a sufficient condition for an appropriate action with respect to that item of knowledge by another person in the absence of any communication between them through sensory channels. Obviously the success of such an experiment does not tell one that 'tele-

pathy' (or ESP or psi) is the appropriate name to apply to the capacity revealed by such an experiment, nor anything further about the nature of the capacity than is implied in the experimental situation. 'Telepathy' (or ESP or psi) is merely the name given to the capacity indicated by the experiment, and new experiments must be designed to discover anything further we want to know about the capacity. There is nothing metaphysical about that; the concepts of parapsychology are parallel to such concepts as 'energy', 'radiation', etc.

It is rather surprising to find that a book of this kind has a foreword by Julian Huxley which expresses a hope that it may be widely read and taken to heart. However, Huxley expresses disagreement with the main thesis of the book, and says that he cannot follow the author 'in stigmatizing studies on telepathy, clairvoyance, etc., as "occult research", unfit to be admitted to our universities' (p. 6).

The real difference between Mr Rawcliffe and the experimental psychical researcher (or parapsychologist) is that between the Inquisition and Galileo. He knows beforehand what can and what cannot happen; the psychical researcher wants to find out by experiment. We can agree with him that we must find a 'rational' explanation of parapsychological experimental results, but that is not necessarily the explanation that would have seemed to us to be the most rational one before experiment started. If ESP proves to take place, then explanation by ESP is not an irrational one. The question must be settled by experiment, and Mr Rawcliffe gives us no new experimental facts and no new insight into the old ones. Our problems are to be solved by experiment and not by the methods of the debating society.

R. H. THOULESS

THE PSYCHIC SOURCE BOOK. Edited by Alson J. Smith. Introduction by Pitirim A. Sorokin. New York, Creative Age Press, 1951. xii, 442 pp. \$4.00.

This book is described by the publishers as 'a big, exhaustive, factual compendium on the ever-alluring subject of extrasensory perception and psychic research'; and even after applying a necessary corrective to a publisher's natural enthusiasm it can be said the contents cover a wide field and should be a useful primer for newcomers to parapsychology.

It consists mainly of twenty articles culled from the writings of eminent scholars and scientists, about half of whom are from this country. These articles are mostly of the review type and, though necessarily condensed, give a fair account of their authors' views.

Thus we have F. W. H. Myers on 'The Daimon of Socrates', J. W. Dunne's 'An Experiment with Time', G. W. Balfour on 'The Ear of Dionysius', Walter Prince on 'Patricia Worth', J. H. Hyslop on 'Mrs Piper', Mrs H. Sidgwick on Professor Gilbert Murray's 'Thought-transference experiments', the Rev. C. Drayton Thomas on 'Book tests', Dr Soal's Myers Memorial Lecture 'The Experimental Situation in Psychical Research', Dr Rhine on 'Precognition'—all taken wholesale from the original articles and making a rich and varied collection.

Introductions and explanatory notes, mostly quite short, have been added by the compiler, the Rev. Alson J. Smith, and by Pitirim Sorokin, Eileen J. Garrett, and J. G. Pratt. These express a dualist view of mind and body and frankly assume that psychical research has now established the reality of a Spirit of Man transcending his material environment. To some this will detract from the book's scientific value as going further than the presented facts warrant. There are no hints of alternative interpretations.

With so vast a field to cover there must be many omissions even in a compendium of 150,000 words. Thus nothing whatever is said of physical mediumship, nor, rather surprisingly, considering the country of origin, of psychokinesis. The bare word, if a rather scanty index is to be trusted, only occurs twice in the text, with no explanation. PK is not included in a useful glossary, though 'telekinesis' is defined as being 'the alleged supernatural movement of objects'. The use of the cautionary adjective is interesting as it is not used in the parallel definitions of telepathy, clairvoyance, precognition, etc., which are defined as established facts. A bibliography is included with over 300 entries among which are Bunyan's *Pilgrim's Progress*—and *The Koran*. There is also a Biography of forty-six eminent personalities in psychical research, and it is pleasant to see E. J. Dingwall's name jostling for a place in this Roll of Honour with Emanuel Swedenborg, Harry Price, and Sir Arthur Conan Doyle.

G. W. F.

NEW LIGHT ON SURVIVAL. By Roy Dixon-Smith. London, Rider, 1952. 328 pp. 21s.

In this book Mr Dixon-Smith has given us a detailed account of his conversion to Spiritualism after the tragically early death of his wife. It is written with great sincerity and enthusiasm, and with the purpose of bringing the same comfort and assurance to others which he has obtained from his experiences with both trance and physical mediums. It is offered not merely as a narrative but

as a serious contribution towards evidence of survival, and it is from this standpoint that it should be judged.

Part I consists mainly of descriptions of the author's sittings with trance mediums, and in his Explanatory Note (page xvii) he claims that 'the case for survival is at this stage considered to be proved'. The reader's first impression is that the author is unaware how much more difficult it has become to obtain crucial evidence of survival owing to our increased knowledge of ESP. Mr Dixon-Smith, however, gets over this by postulating that the *modus operandi* between a discarnate mind and that of the medium is a form of hypnosis; but that the possibility of this process taking place between medium and sitter is excluded by the latter closing his subconscious mind 'as tightly as possible by will and auto-suggestion' so that no medium 'can hook willy-nilly on to one's subconscious memory' (p. 45). The author's attempts to prove this statement are most unconvincing and can hardly be taken seriously. There is strong evidence that telepathy between sitter and medium does occur. (A typical example can be found in the S.P.R. *Journal* for February 1948 entitled 'Emergence of an apparently Pseudo-Communicator' by Mrs Heywood.) This should not be considered a more 'extraordinary super-human power' (p. 45) than that which the Spiritualist hypothesis entails.

An objective study of Mr Dixon-Smith's sittings reveals that a great deal of the information given to him through mediums was already known to him and was given in private sittings at which he took his own notes (p. 62) and in certain cases had his hand on a ouija board with that of the medium (p. 113). The author's honesty is not being questioned, but this type of evidence does not reach the standard required in psychical research. Proxy evidence is far more valuable and he gives some interesting cases, but he does not draw sufficient distinction between their evidential value. Accurate information given to a proxy sitter is more impressive when neither absent nor proxy sitter are known to each other, and an involuntary leakage of information or an unconscious memory cannot possibly play a part. It is true, however, that it is the *accumulation* of evidence of personality which often brings conviction to a sitter, and such evidence cannot be entirely disregarded even though it may not come up to scientific standard.

Mr Dixon-Smith rightly reminds us of the famous cross-correspondence cases, and others of high evidential standard published by the S.P.R., where an explanation based solely on ESP is inadequate and postulates a degree of that faculty which is at present beyond our knowledge. It is the existence of cases of this standard which makes further and wider research of such importance, but it is imperative that it should be carried out under

conditions of the most carefully devised control (however unassailable the good faith of the experimenter may be) and if possible in conjunction with modern psychological research.

This applies with even greater force to the physical phenomena described in Part II. They cover every form of manifestation and are so remarkable that it is to be regretted that more details are not given regarding the methods of control. This branch of inquiry is far more subject to fraud and malobservation than the author appears to realize, and it is to be hoped that Mr Dixon-Smith will give us more detailed and corroborated descriptions of his sittings in the next book which he has promised us.

The absence of an index is a great inconvenience.

K. A. G.

CORRESPONDENCE

QUALITATIVE MATERIAL AND THEORIES OF PSI PHENOMENA

SIR,—Many members of the Society may, like myself, doubt their ability to co-ordinate the facts of psychical research with physiological science or to bring them within any metaphysical scheme. If so, they will share my gratitude to Dr J. R. Smythies for his paper in the *Journal* for September 1951, and to their fellow-members who have commented on it in the issue for January 1952. The kind of theory which would be most helpful to us would be one that covered as many as possible of the facts of psychical research which have been established, without bothering about material that at the present time needs support from the results of further research.

The literature of psychical research, in this and other countries, is enormous. It would be impracticable for any intending theorist to read every report in every publication with the attention necessary to form an opinion whether the occurrences there described were properly attested, and whether in each case there were adequate grounds for dismissing sensory clues, chance, and other 'normal' explanations. Should he succeed in completing such a task he would have little leisure for theorising.

Fortunately, in assembling material to be brought under his survey, he can profit by the labours of his predecessors in this Society. His concern will not be with separate, individual, occurrences, least of all with instances, whether experimental,

mediumistic, or spontaneous, which present peculiarities that cannot readily be paralleled from other reported instances, themselves well attested. Exceptional instances are treacherous ground, and he will be safer if he keeps to classes or types of phenomena that are represented by several examples published in *S.P.R. Proceedings*.

The long life of the Society, its freedom from corporate dogma, and the enterprise of its members in research are a pretty good guarantee that every sort of phenomenon will be investigated by a member and reported to the Society. But no report will appear in our *Proceedings* until it has been screened by the Committee of Reference, which dates back to the very early days of the Society and has numbered many men and women of outstanding ability, critical judgment, and varied opinions.

It would be absurd to claim that our investigators or the Committee of Reference have made no mistakes. A few hoaxes have succeeded, and examples of carelessness in experiment, observation, or record are to be found. But when several examples of about the same type of supposedly paranormal occurrence are to be found reported in our *Proceedings*, that type has, I suggest, a *prima facie* claim to be included in any theoretical scheme.

Should the intending theorist share the doubt, which is sometimes expressed, as to the possibility of deciding whether such qualitative material as spontaneous cases, mediumistic 'communications' and so on can be attributed to chance or not, and does not trust his own judgment, he can always seek the guidance of notable sceptics, such as Mrs Sidgwick and Miss Alice Johnson, whose critical abilities were exercised over most of the field of psychic experience. I do not believe he will anywhere find a more objective criterion for matter worth including in a theoretical scheme than publication in *Proceedings* combined, where qualitative material is concerned, with acceptance as probably paranormal by these experts. This will give him quite as much material to work on as he is likely to need or desire.

Now, if this yardstick be applied to the papers of Dr Smythies and his critics, it will be noted that, while in some respects the material discussed by them lies outside the limits suggested above, there is a very large quantity and variety falling within those limits which most of the writers neglect. With few exceptions they confine their remarks to quantitative experiments. Some of these, notably the Soal-Goldney experiments, thoroughly deserve their attention. Others might conveniently wait until we have a larger quantity of positive results obtained under good conditions.

If the results of quantitative experiment were typical of psychic phenomena as a whole, there would be no occasion to press a

theorist to travel outside them. But for reasons too well known to readers of the *Journal*, arising from their being experimental and quantitative, that is far from being the case. If, on the other hand, there were no points of contact between the quantitative and the qualitative, it might be simplest to get two teams on the job in the assurance that their theories would not conflict or overlap. But as things are, attempts to frame a theoretical scheme exclusively out of quantitative material may be quite misleading if it is assumed to be applicable to other types of phenomena, and even misleading as regards the quantitative material itself, as obscuring its connexion with and partial resemblance to other material left outside the scheme.

It is gratifying to note that our American colleagues are alive to the importance of qualitative material. I would refer in particular to the short paper by Dr Emanuel Schwartz in the American Society's *Journal* for January 1952, 'The Psychodynamics of Spontaneous Psi Experiences'. Referring to a previous article by him (October 1949) he writes (p. 3) :

I suggested that over the period of the last seventy-five years the original emphasis in psychical research [a footnote here mentions Myers's *Human Personality*] had been shifted from the *experiencers* to the *experiences*. It seemed to me that psychical research divorced from the study of human personality was a meaningless undertaking and that the cul-de-sac into which psychical researchers have been led might have been anticipated.

The general purpose of the article is to stress the need for the study of motivation in psychic experiences and of the 'interpersonal field', to use Professor Gardner Murphy's phrase. With this object and with several passages in Dr Schwartz's article I am in full sympathy. When, however, he speaks (p. 3) of the need to shift the emphasis 'away from the "phenomena" and their veridicality back to human beings', I think a source of possible misconception is to be noted.

Let us take two imaginary cases. (A) Jones has a waking vision of Smith being involved in a motor smash : Smith has not in fact been so involved. (B) Jones has a similar vision at the moment when Smith was in a motor smash a hundred miles away, the attendant circumstances being so accurately reproduced in the vision as to make an explanation by chance-coincidence highly improbable. (How convincing these imaginary cases are!)

It would not, I suggest, be at all safe to assume that the interpersonal relation between Jones and Smith was the same in the two cases. It might be that the motivation of case A was an emotion of Jones regarding Smith that was not reciprocated, and

in case B an emotion that was. Whether that could be shown or not, the difference between the two cases suggests a probable difference in the psychological situation. It seems to me therefore that an analysis of the psychology of spontaneous cases (and of mediumistic 'communications' also) will lose much of its value unless the laborious first step be taken of sorting out the material under examination into veridical and non-veridical. From his praise of the early investigators, who tested each case very thoroughly to see how far it could be counted as veridical, I should infer that Dr Schwartz himself is under no misconception as to the need for this, but some of his remarks might perhaps mislead incautious readers of his article.

W. H. SALTER

THE SHACKLETON EXPERIMENTS: MR RAWCLIFFE'S THEORY OF DOUBLE WHISPERING

SIR,—In *The Psychology of the Occult*, Mr D. H. Rawcliffe puts forward a theory of double whispering to account for the results of the experiments with Basil Shackleton. Students of psychical research will realize that the theory is so preposterous as to be scarcely worth refuting, but for the sake of uninformed persons who may read the book it may be advisable to make a few comments.

The author supposes that the experimenter, in addition to presenting the random numbers at the aperture in the screen and calling in a loud voice the serial numbers 1-25, involuntarily whispers the *next* number on his list or tags on its initial consonant to the serial number. The Agent sitting on the other side of the screen picks up this whisper, decodes it into the corresponding animal's name, and whispers the name or its initial letter, and the whisper is picked up by Shackleton sitting 18-20 feet away in the next room. All this complicated series of exchanges is supposed to be packed into the $2\frac{1}{2}$ seconds between successive calls. When, however, the experiment is speeded up to twice the normal rate, the experimenter is supposed instead to whisper the *next number but one* on his list, and this is said to account for the (+2) displacement. This double whispering and decoding has now to be compressed into an interval of $1\frac{1}{2}$ seconds or less!

Shackleton succeeded brilliantly with no less than seven different experimenters. All these, according to Mr Rawcliffe, must have been whispering the number one or two places removed from the one they were actually showing at the aperture, and all of them must have been completely unaware of it!

In any case, in putting forward this theory Mr Rawcliffe omits to say that true involuntary articulation occurs as a reinforcement effect and not as a deliberate separate process. He is therefore in effect saying that the experimenter (1) looks up at the appropriate random number on the list, (2) deliberately shifts his glance to the next number or the next but one, (3) looks down to pick up the card bearing the appropriate number, (4) presents it at the aperture, (5) calls out the next serial number, and (6) deliberately whispers the next random number or the next but one. All this, plus the picking up and decoding of the whisper by the Agent, and the latter's own whispering, would indeed constitute a phenomenon of the first order, even if not compressed into a maximum of $2\frac{1}{2}$ seconds! Of the implication of fraud, I will only say that ten different people—seven experimenters and three Agents—would have had to be party to it!

There are other phenomena—e.g. the success of one Agent and the total failure of the other when two agents were employed simultaneously, either with the same or with different targets—which Mr Rawcliffe's theory is incapable of accounting for, but to deal with these would be a waste of your space and of the reader's time.

I shall scarcely be expected to comment here on Mr Rawcliffe's remarks on the conduct of the Antwerp-London experiments with Mrs Stewart.

S. G. SOAL

ESP AS GUESSWORK

SIR,—In two recent broadcast talks on 'The Significance of Parapsychology', and in the conflated version of these which I gave to the Society in February, I suggested that if we thought of ESP not as a species of either cognition or perception but as a kind of guesswork, we might be led to investigate ordinary guesswork; and that what we found out might throw into relief significant peculiarities of paranormal guesswork. I have been asked to expand and explain this suggestion. I am glad to be driven to try to do this: for what I originally had in mind was an extremely hazy and tentative idea; and it is good to be forced to find what—if anything—can be made of it.

Obviously the first difficulty lies in the notion of ordinary guesswork. For—at least in the present state of our ignorance—there is no way to distinguish decisively in any *single* case between a guess (or hunch or dream or vision) which just happens to be right and a correct guess (or hunch or dream or vision) in which the

ESP factor is operating. This is because all we can at present mean by the term 'ESP factor' is the factor which gives rise to significant deviations from pure chance expectation in a *series* of guesses (or whatnot). If we use the term to mean more than this, then the evidence at present available has not established the presence of an ESP factor: if 'ESP' is taken to refer to the putative unknown means or mechanism by which successful ESP runs are achieved, then there are no sufficient grounds for thinking genuine ESP occurs; for though conjuror's means and mechanism can produce *bogus* ESP effects, we have no reason to suppose *genuine* ESP runs are produced by any means or mechanism; and every reason to suppose the contrary. (What means or mechanism *could* operate backwards to produce genuine precognitive ESP?) The difficulty could be got round, at least temporarily, by prescribing that guesswork (and whatnot) was ordinary when and only when it was done by someone who has always failed to score significantly in ESP tests. This is no doubt excessively rigorous; but it's as well to be on the safe side, by thus excluding the dud runs of the sometimes successful.

Now what I mean by studying ordinary guesswork (and whatnot) is finding out why people guess one thing and not another (dream one thing and not another, have one particular hunch or vision and not another) when no ESP factor is involved. If we knew more about this it might be easier to see more of the nature of this mysterious ESP factor, which appears occasionally to upset the ordinary mechanisms and motivations of the normal. What leaps to mind here are Freud's masterpieces: *The Psychopathology of Everyday Life* and *The Interpretation of Dreams*. But a great deal of other work has been done; and much more needs to be done.

Now I have developed the idea, I am no more sure than I was when I first made the suggestion that there really is anything in it. But perhaps, even if this is just one more dead end, it will have been worthwhile to have reminded psychical researchers—especially those working on putative spontaneous phenomena—that Freud did show that very many apparently paranormal predictive omens and dreams could be sufficiently explained in terms of the motives of the dreamer.

ANTONY FLEW

'THE CLAIRVOYANT THEORY OF PERCEPTION'

SIR,—I should like to make two comments on Captain Moncrieff's letter in the March issue of this Journal.

(1) I did not say, as Captain Moncrieff suggests, that his theory cannot account for disordered vision in cases of cataract. In fact, I expressly said that it could account for this but not for disordered vision in cases of disease of the optical apparatus *proximal* to the retina, i.e. in the optic tracts, optic radiations, and occipital and parietal cortex. Captain Moncrieff explains the various phenomena and syndromes which I mentioned thus: '... if clairvoyance is a mental faculty, such abnormal cerebral conditions as above would naturally be expected to produce them. ... Thus, in case of "vascular lesions of the parietal lobe" we would hold that their effect on vision may possibly be due to the lack of integration or correlation in respect to the cerebral processes.'

Now the disorders of vision caused by such vascular lesions are fairly extensive, and the visual field is grossly disorganized. Thus the integrative and correlating processes, which are held to fail in these cases, must be very greatly responsible for the orderly presentation of the normal visual field. Furthermore, experimental psychology has shown that the actual size of objects in the visual field and the spatial relations between these objects is quite different from the size and relationship of the objects of the common physical world. This is particularly so in the case of distant objects. If the actual nature of the visual field is borne in mind, together with the evidence as to its means of presentation that we can actually inspect—after-images and the stroboscopic phenomena—it seems that these integrative and correlating processes in themselves provide an adequate explanation of the missing link in perception. The visual and auditory fields and the somatic sensory field making up the perceived body must actually be constructed by mechanism—that is by process in extension. Whether this mechanism is in the brain or in the psyche is not immediately relevant. The problem of externality of sensa, which Captain Moncrieff's theory attempts to solve, is an entirely false problem due primarily to the confusion in common sense between the perceived body and the physical body and to the confusion in neurology between the perceived body and the body-image. All sensa, including the visual fields and the perceived body, are wholly *inside* the human organism—geographically, anatomically, and physiologically *inside* it.

(2) I would not agree that 'no theory can be expected to provide a full and adequate explanation both in respect of normal and abnormal conditions of vision'. There is no clear-cut

division in nature between the normal and the abnormal, and any theory which cannot account for both is inadequate.

In conclusion, I would like to suggest for further reading Paul Schilder's book *The Image and Appearance of the Human Body* (London, Kegan Paul, 1935). Further advances in philosophy depend largely upon the integration into the existing philosophical structure of the large body of evidence contained in the special humanistic sciences of neurology, neurophysiology, experimental psychology, and neuro-anatomy.

J. R. SMYTHIES

OBITUARY

J. G. PIDDINGTON

WE regret to record the death, on 1 April at the age of 83, of Mr J. G. Piddington. A member of the Council for many years, he was elected a Vice-President in 1921 and President in 1924. He contributed several papers to the *Proceedings* on the Cross-correspondence scripts, and was Trustee of the Research Endowment Fund for many years. An obituary notice will be published in the *Proceedings*.

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